Peer-review report of

Harms, M., Bruens, A., Jones, J. & Ligon, G. (2026). Deadly persuasion: A multilevel analysis of leader influence tactics and lethality in violent extremist organizations. *advances.in/psychology*, 1, e165880. https://doi.org/10.56296/aip00069

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Round 1

Editor

Date: 2026-02-17

Dear Authors,

Thank you for submitting your manuscript, “Deadly Persuasion: A Multilevel Analysis of Leader Influence Tactics and Lethality in Violent Extremist Organizations,” for consideration in our special issue, *Advances in the Psychology of Violent Extremism*. We have received feedback from two experts with significant expertise in this area, and we appreciate their constructive comments. As an editorial standard, we both assessed the manuscript before reading the evaluations. These evaluations, in conjunction with our own editorial review, have informed our decision.

There is a consensus that your paper is highly innovative, particularly in its theoretical application to VEOs and its subsequent empirical testing. For instance, Reviewer 1 states, “The paper addresses an important and timely research question and is highly original in its integration of organizational psychology, leadership theory, and radicalization studies.” Similarly, Reviewer 2 states, “I enjoyed reading this paper, I think it was well-written, and I think it studies a topic that is interesting and worthy of publication in this outlet.”

While both reviewers offer various suggestions for improvement, they agree that your work has the potential to make a significant contribution to the field and to our special issue. We strongly share this assessment. Therefore, we invite you to submit a revised manuscript that thoroughly addresses the issues highlighted by the reviewers and us, the editors. The reviewers devoted considerable effort to providing thoughtful feedback; while we ask that you respond to each of their points, we will not reiterate them in full here. Instead, we wish to highlight overarching issues that we and the reviewers consider critical for revision.

Echoing Reviewer 1, we request a stronger rationale for your chosen frameworks. While you clearly present the three leadership styles and power orientation, further explanation is required regarding (a) why the specific combination of these two factors is important, and (b) why they are particularly relevant to VEOs. What theoretical mechanisms link this combination to lethality and sustainability? Even though the study is exploratory, specific design choices, such as your test of three-way interactions, suggest underlying guiding concepts and ideas. Please add a section outlining this theoretical grounding, placed before the research question section.

Relatedly, we believe the theoretical framing requires further development overall. At present, it is not entirely clear how and why differences in leadership style are expected to translate into differences in lethality or sustainability. The manuscript suggests that personalized versus socialized leaders should differ in performance over time because of how they frame the broader mission. However, the direction of these differences and the mechanisms driving them are not clearly articulated. For instance, are socialized leaders expected to produce greater lethality because they foster stronger collective identity and willingness to sacrifice? Or are they expected to generate more sustainable performance because they distribute power and reduce

fragmentation? Conversely, might personalized leaders produce greater volatility, short-term spikes in violence, or internal splits? These expectations are not explicitly specified. While the argument is plausible, it is not yet theoretically tight.

Like Reviewer 1, we also question the decision to treat time solely as linear. Please test alternative models to determine which shape fits the data best. Furthermore, the reviewer raises a valid point regarding whether lethality is indeed an indication of success or failure. Reviewer 2 similarly states, “that there are organizations for which a smaller number of deaths (surgical killings) may be indicative of a successful group.” We agree that your paper would be strengthened by a discussion of both points.

In addition, we encourage you to clarify how lethality is attributed. In some movements—for example those associated with so-called “leaderless jihad”—attacks were carried out by individuals who were ideologically aligned but not organizationally directed. Similarly, lone-actor extremists may be inspired by a group without being operationally connected to it. Please clarify how such cases are handled. If these attacks are counted as part of an organization’s lethality, this may inflate the apparent effect of leadership, since some violence may not be directly leader-driven.

Reviewer 2 raises a critical point regarding the omission of the final number of leaders included in the data, as well as the distribution of leader types. This information is essential for assessing the statistical power of your study. A total of 60 organizations coded into six cells intuitively suggests a potentially underpowered design (roughly n = 10 per group, assuming equal sizes), although this also depends on the number of time points within each organization (please report this information; see also comments by both reviewers). We need to establish the study's power to identify effects. Please provide a sensitivity analysis to identify the minimum detectable effect given your design and the number of Level 1 and Level 2 units. Essentially, testing up to a three-way interaction (leadership style x power orientation x time) is most likely underpowered, as Reviewer 2 notes. We agree with Reviewer 2 that you should focus only on two-way interactions unless you increase the sample size (see next comment).

Reviewer 2 makes critically important observations regarding your analytic strategy, which align closely with our own independent assessment. We request that you re-specify all models to include all constitutive lower-order terms regardless of their statistical significance (all main effects and, where applicable, two-way interactions as missing in Table 1; note however that you likely are underpowered for tests of three-way interactions, which should be omitted unless you increase the sample size). Excluding these terms violates the principle of hierarchical completeness and distorts the interpretation of higher-order effects (see Reviewer 1 for a similar comment).

For instance, under the section “Leadership Style and Performance,” please confirm that both the main effect of time (not reported) and leadership style (reported) were included when testing the cross-level interaction and report stats for both. This requirement also applies to the analyses under “Power Orientation and Performance.”

Similarly, for the three-way interaction between Leadership Style, Power Orientation, and Time, the current model omits the main effect of Power Orientation and the relevant two-way interactions. This omission forces the variance of these terms into the higher-order interaction, potentially invalidating the findings. Please re-run all analyses throughout the manuscript using fully hierarchically complete structures to ensure that the reported significant interactions are not artifacts of these exclusions.

In addition, many tests and comparisons currently lack a formal significance test yet are interpreted at face value (see a similar comment by Reviewer 1). Please consistently present exact three-digit *p*-values, except when $p < .001$. Provide *p*-values for all reported effects, including those on lines 289–290, 301–302, and 335–352, as well as in Table 1 (provided the three-way interaction is well powered and can be kept). For all group comparisons, present an effect size estimate, such as an MLM-based Cohen's *d* (calculated by dividing the effect of interest by the residual standard deviation of the model). Please also clarify in the text that the Greek beta reflects standardized effects. Finally, please report 95% confidence intervals (CIs) instead of standard errors throughout the manuscript.

Splitting the sample to test for moderation is suboptimal, particularly given the small number of Level 2 units in your study. Most modern statistical packages allow for the decomposition of three-way interactions without the need to estimate separate models to test two-way interactions. Also, in your current separate models, you report two-way interactions without presenting full statistics, including *p*-values. Furthermore, as Reviewer 2 highlights, graphing the slopes of the three-way interaction would significantly increase the interpretability of the effects; please present such a graph in the revision.

It is also unclear why you did not control for obvious factors such as organization size, which is expected to relate to lethality (see Reviewer 1 for the same point, and Reviewer 2 for related points). Please include this control in your revision. Other important variables available in the dataset, such as ideology, goals, and potentially group processes, should also be included. Lethality is also shaped by contextual factors such as conflict intensity, state capacity, target selection, territorial control, organizational age, alliance structures, and counterterrorism pressure. It is not clear whether and how such factors are controlled for. If not included, please justify their omission or incorporate them in the revised models.

Relatedly, we are not fully convinced that leadership style necessarily operates in the same way across the different types of organizations included in the sample. The dataset combines religious, ethnonationalist, and politically motivated groups. However, concepts such as martyrdom, sacrifice, and transcendence may function differently across ideological contexts. For example, martyrdom rhetoric is deeply embedded in certain religious extremist movements. In such cases, what is coded as "socialized" leadership may align structurally with the broader ideological framework of the organization. This may not be the case for ethnonationalist or secular political movements. If all ideological categories are pooled without testing interactions between ideology type and leadership style, important differences may be obscured. At minimum, we would expect robustness checks where leadership effects are examined as moderated by ideological categories.

We fully agree with Reviewer 2 regarding the necessity of disclosing exactly which organizations were selected. This level of transparency is critically important for evaluating and contextualizing the results and to understand the balancing you describe. This brings us to the sampling strategy, which is currently not described in sufficient detail. It remains unclear how sampling 60 VEOs helped “control for factors beyond the scope of this research that may impact differences in lethality and longevity across groups.” We assume you balanced all factors so that they were similar across each cell, but no information has been provided to verify this (provide statistical tests that should show non-significant differences). Please provide an overview of the sampled organizations and their characteristics in the supplementary information.

Finally, it is not clear why the full dataset containing 280 VEOs was not utilized. Was the selection of 60 due to feasibility constraints (e.g., the workload involved in coding)? Using the full sample of 280 would likely alleviate power concerns and mitigate the risk that the findings for the 60 VEOs do not represent the broader population. If you utilize the full sample, you can statistically control for geographical, ideological, and other factors, rather than relying on imperfect balancing.

We also have concerns regarding the decision to drop leaders from the candidate list in cases of coding disagreement. While this approach may improve reliability, it risks introducing selection bias by excluding precisely those cases that may be theoretically central in VEO contexts. Please justify this decision more explicitly and consider robustness analyses that retain ambiguous cases.

In addition to the major issues outlined above and the other points by the reviewers, please address the following specific points regarding different sections of the manuscript.

INTRODUCTION

Please present previous research that justifies treating the different leader types (charismatic, ideological, pragmatic) as mutually exclusive categories.

METHODS

Please provide a justification for the sampling timeframe (1970 to 2020), as it currently appears somewhat arbitrary.

Please explain the process and personnel involved in determining that an organization “(1) was the most prolific (activity, notoriety, growth) (2) maintained a relatively stable organizational structure, and (3) had a single, identifiable leader over the organization.” How were these decisions validated? Criteria (1) and (2) appear more subjective than (3), and readers require transparency regarding the procedures you followed.

Additionally, how is the data handled when a leader dies of unnatural causes (controlling for age)—a not uncommon occurrence in VEOs? That seems indicative of failure.

Please make the data and analysis code openly available for reproduction during the review process. Please also explain the specific nature of the “third party restrictions prevent this” if full availability is really not possible. As we understand it, you have analyzes publicly available data?

Finally, please specify the software, libraries, and versions used for all statistical analyses.

DISCUSSION

There appears to be a contradiction between your results and your discussion. In the results, you reported the following: “Additionally, the results suggest that the change in lethality of an organization remains relatively stable over time for both charismatic leaders (β = -.03, *OR* = .97) and ideological leaders (β = .02, *OR* = 1.01), but increases substantially for pragmatic leaders (β = .20, *OR* = 1.21). This is in line with research that suggests pragmatic leaders exert influence iteratively over time and tend to learn from previous actions to improve upon future iterations (Mumford, 2006).”

However, in the discussion you state: “Additionally, pragmatic leaders demonstrated the least sustainable, less lethal performance, aligning with prior research suggesting that pragmatics tend to have less radicalized followers due to their more logical and rational influence style.” How does the finding that pragmatic leaders increase in lethality over time align with the assertion that they demonstrate “less lethal performance”? Please reconcile these statements.

At one point, you say that the unequal leader distribution was expected based on what is known about VEOs, but you do not cite any work supporting this. Please do so.

STYLISTIC ISSUES AND REFERENCES

Page 38: Please change “and highlights” to “highlighting.”

Please change “underresearched” to “underutilized.”

The sentence on l. 210-211 appears incomplete.

Line 237: Please change the citation to “Strange and Mumford (2002)” (APA 7 only uses the ampersand when citing two authors within parentheses).

Line 409: Please remove the duplicate word “symbolic.”

DOI numbers for all journal articles must be included and formatted as URLs in accordance with APA 7 guidelines. There are also missing references, incorrect years, and similar errors. Please upload your document to our free tool at https://apa.advances.in to identify and fix these issues, and then re-upload to validate. Please note that the tool is probabilistic and may flag false positives, which you can safely ignore.

In summary, we believe this manuscript holds considerable promise, but its potential has not been fully realized in its current form. We therefore invite you to submit a revised version that addresses the points outlined above. Please submit your revision by April 30. Should you require additional time or wish to discuss how best to address these comments, do not hesitate to contact us. We look forward to receiving your revised work.

Best regards,

Jonas R. Kunst & Milan Obaidi

Reviewer 1

This manuscript examines the relationship between leadership styles, power orientation, and the lethality of violent extremist organizations during their period of peak influence. The paper addresses an important and timely research question and is highly original in its integration of organizational psychology, leadership theory, and radicalization studies. The use of historiometric

methods is particularly interesting and has the potential to make a meaningful contribution to the literature. Overall, I find the manuscript promising. However, I have several concerns regarding the theoretical framing, analytic strategy, and clarity of reporting that should be addressed before the manuscript can be considered for publication.

1. Introduction and Theoretical Framing

The authors introduce two leadership frameworks—CIP and power orientation—but the rationale for privileging these models over competing leadership theories remains underdeveloped. The introduction would benefit from a more explicit discussion of alternative leadership models and a clearer justification for why CIP and power orientation are especially suited to explaining organizational lethality in violent extremist contexts. Clarifying whether these two approaches are intended to be complementary or theoretically distinct would also strengthen the conceptual foundation of the study.

2. Methods

- **Operationalization of Leadership Styles**
  The definitions used by judges to code leadership styles appear related to, yet not fully aligned with, the descriptions of leadership styles provided in the Introduction. It is unclear whether these discrepancies are intentional (e.g., due to data limitations or historiometric constraints) or inadvertent. The authors should explain this divergence and ideally harmonize the conceptual and operational definitions to improve construct validity.
- **Interrater Agreement and Case Exclusion**
  The manuscript notes that some leaders were excluded due to insufficient interrater agreement. It is unclear whether this resulted in the exclusion of entire organizations and, if so, how many organizations were dropped. The authors should clearly report (a) how many leaders and organizations were excluded, (b) how many remained in the final analytic sample, and (c) the level of interrater agreement achieved after exclusions.
- **Model Selection and Fit Procedures**
  The authors state that non-significant effects were removed based on model-fit procedures, but no details are provided regarding the criteria used, the sequence of model comparisons, or the results of these procedures. This information is essential for evaluating the appropriateness and transparency of the analytic approach.
- **Modeling of Time**
  I have concerns about the decision to model time as a strictly linear predictor. Given the developmental nature of organizations and the concept of a “height of power,” it seems plausible that lethality may follow non-linear trajectories (e.g., quadratic or U-shaped patterns). The authors should consider alternative specifications of time or, at minimum, justify the linearity assumption more explicitly.
- **Specification of the Final Model**
  The final analytic model is not clearly described. The authors should provide a full specification of the final model, including all fixed effects, interaction terms, and random effects, so that readers can fully understand and potentially replicate the analysis.

3. Results

- **Association Between Leadership Style and Power Orientation**
  The manuscript does not report the degree of association between leadership style and power orientation. Providing this information (e.g., cross-tabulations or measures of association) would help readers assess whether these constructs capture distinct dimensions of leadership or whether there is substantial conceptual and empirical overlap.
- **Descriptive Information on "Height of Power"**
  Please report the average duration (in years) of the "height of power" period across organizations, along with the standard deviation. This would provide important context for interpreting the time effects in the models.
- **Use and Interpretation of Multiple Models**
  The Results section appears to present three different models:
    1. A model focused on leadership style,
    2. A model focused on power orientation, and
    3. A combined model including both predictors and their interaction with time.

However, only one model is theoretically justified in the Methods section. Leadership style is significant in the first model but not in the combined model; power orientation is not significant as a main effect in the second model, and its status in the combined model is unclear. The combined model includes a significant three-way interaction between leadership style, power orientation, and time. The authors should clarify the rationale for estimating and reporting three separate models. I would recommend either focusing on the two simpler explanatory models or exclusively on the combined model; using both simple and combined models does not currently appear theoretically or analytically justified.

- **Interpretation of the Three-Way Interaction**
  To interpret the three-way interaction, the authors conduct additional two-way interaction analyses between power orientation and time for each leadership style. However, these results are reported without confidence intervals or p-values (p. 17), making it difficult to assess statistical support. Proper inferential statistics should be included.
- **Table 1 and Reference Categories**
  Table 1 reports main effects of leadership style and the three-way interaction terms, but it is unclear whether this represents the full model. Because the model was not fully specified in the Methods section, the table is difficult to interpret. Moreover, based on the confidence intervals, it appears that only the Pragmatic × Socialized × Time interaction is statistically significant. Including p-values would make this clearer.

Additionally, while Pragmatic leadership appears to be the reference category for the main effects, the reference categories for the three-way interaction terms are unclear. It appears that the authors may have changed reference categories and rerun the model multiple times to obtain the reported interaction terms, which is unconventional. With Pragmatic leadership as the reference category for

leadership style and, for example, Personalized power orientation as the reference category for power orientation, I would expect only two three-way interaction terms:

- Charismatic vs. Pragmatic × Socialized vs. Personalized × Time

- Ideological vs. Pragmatic × Socialized vs. Personalized × Time

- **Post Hoc Contrasts**
  Given that only the Pragmatic × Socialized × Time interaction appears to be significant, I do not believe it is appropriate to conduct post hoc contrasts involving leadership categories that are not implicated in this interaction.

4. Additional Predictors

Organizational size (e.g., number of members) is likely to be an important determinant of lethality and may confound the observed relationships. The authors should consider including organization size as a covariate or, at minimum, discuss its potential influence.

Discussion

The Discussion raises important points, but the interpretation of lethality as a performance indicator warrants further reflection. It is plausible that organizations resort to increased violence when they are failing to achieve political goals rather than when they are most successful. This alternative interpretation should be more thoroughly considered and integrated into the discussion of findings and their implications.

In sum, this is a creative and potentially impactful manuscript with a strong interdisciplinary premise. However, substantial revisions are needed to clarify the theoretical justification, improve transparency and rigor in the analytic approach, and ensure accurate interpretation of complex interaction effects. I encourage the authors to address these concerns, as doing so would significantly strengthen the manuscript.

Reviewer 2

This research examined the influence of leadership style (using the CIP model) and power orientation of leaders of extremist organizations on lethality of these organizations. Analyses used an LMM approach. Results from three models were reported: one modeling an interaction between time and leadership style, one modeling an interaction between time and power orientation, and one modeling a three-way interaction.

Generally speaking, I enjoyed reading this paper (which is not something I can always say). The paper was well-written, easy to follow, the introduction did a nice job laying out the theoretical grounds for the analyses, and the measures were all appropriate for testing the proposed research question.

The paper would be improved with a more thorough reporting of information about the sample, descriptive statistics, and analyses. Below are some specific suggestions and observations:

- It is reported that 56 leaders were included (p. 11). Later, it is stated that cases were dropped when researchers disagreed about which leadership style each on represented (p. 12).

The paper does not state how many cases were dropped, and I don't believe the final numbers were ever updated/provided after dropping cases.

- Interrater agreement was reported for leadership style, but not power orientation. What process was used here? If interrater agreement was not calculated, why did the coding process differ from what was used with leadership style?

- The paper never reports what number of leaders were categorized into each of the leadership styles or power orientations. This is critically important. The sample size is small, particularly for a three-way interaction, and it is possible that cases in each cell of the analyses (i.e., number of pragmatic-personalized leaders) are too small to justify the most complicated model.

- No information is provided about the time/year variable. What was the range of how many years organizations were dominant for? Mean number of years for which each leader was dominant? I imagine the number of years differs for every organization. Were there a small number of highly active groups that could skew the findings? How could this impact interpretation of the findings?

- More information could be provided about the models that were specified. What were the specific random and fixed effects in each model? I assume data were nested within leader, by including a random intercept? Was time modeled as both a fixed effect and a random intercept or slope? Why does the results table for Model 3 (3-way interaction) not provide information on all two-way interactions that should also be in the model? Tables reporting the full results of Models 1 and 2 would be helpful, even if only relegated to supplemental materials.

- A visualization of the three-way interaction would go a long way to improve interpretation of the findings.

A few other questions I have:

- The paper made an explicit point to ensure that the data were selected so that they were evenly selected from hierarchical vs. cell-structures organizations and Western vs. non-Western organizations. I was surprised that analyses never examined these variables as moderators.

- The paper mentions that different ideologies were represented, but never specifies what those ideologies were or how many of each were represented. This seems important to the project. Leadership is defined in ways that seem to be organization based. What I mean by that is that some organizations seem to have cultures that might promote a specific type of leader, and it is possible that one's leadership style is really indicative of an organization's goals vs. the individual style of a leader.

- Not every extremist organization strives to inflict maximal casualties. Sometimes, the political goals of an organization might be better served by minimizing deaths or engaging in surgical killings of specific targets. In these cases, a smaller number of deaths would not be indicative of a poor-performing group. And it is possible that groups that operate this way favor a certain leadership style that would skew the interpretability of the results. I know a version of this is referenced in the discussion as a limitation, but I still thought it worth to state here.

- Is a three-way interaction model appropriate and interpretable given the small sample size? Assuming the full sample of 56 leaders (but some leaders were dropped after coding, p. 12), and assuming an equal distribution of leadership styles and power orientations (which we know did not occur, as the paper states that there were fewer pragmatic leaders than others , p. 20), we are talking about 9 cases per cell. That seems small for estimating such complex effects. I wonder if the three-way model is even appropriate, and whether relying on only the two-way interaction models would be sufficient?

- Does the three-way interaction model demonstrate better fit than the other models? A reasonable approach might be to use a stepwise approach of gradually increasing model complexity (instead of reducing it) to examine if adding additional terms/complexity actually improves model fit as measured by AIC/BIC – i.e., does stepping up from a model with only main effects to two-way interactions to three way interactions improve model fit?

To reiterate my thoughts, I enjoyed reading this paper, I think it was well-written, and I think it studies a topic that is interesting and worthy of publication in this outlet. However, the use of such a complicated model given the sample size makes me uneasy, and the paper in its current form would need significant revisions to more comprehensively report the method and results.

Round 2

Editor

Date: 2026-06-07

Dear Authors,

Thank you for submitting your revised manuscript to our special issue. We are genuinely pleased with how the work has developed; the paper has improved considerably, and the thorough reanalysis of your data, in particular, has done a great deal to sharpen its contribution. It is clear that you took the previous round of comments seriously, and the result is a stronger, more compelling manuscript. The remaining issues are largely presentational, with only a few substantive points still needing attention, and we want to be transparent about our intent in raising them: our sole goal is to help you make this paper as strong as it can possibly be, so that it reaches the significant impact we believe it deserves. In your revision, please address the reviewers' comments together with our own, noted below. As before, please include an itemized response letter explaining how each point was handled, with accompanying excerpts of the revised text. Your previous letter was detailed and of great help.

ABSTRACT

We agree with Reviewer 1 that the abstract is far too general, which limits the paper's impact. First, set up the gap (e.g., "Research on violent extremist organizations (VEOs) has largely treated lethality as a property of single attacks or of organizations in aggregate, leaving open how deadliness shifts across a leader's tenure. The multilevel methods suited to that question remain uncommon in the field."). Then add a bridge (e.g., "We address this by applying two models of outstanding organizational leadership, charismatic-ideological-pragmatic (CIP) influence and

personalized-versus-socialized power orientation, within a multilevel model that tracks annual lethality across 67 VEO leaders and 645 leader-years."), making sure to name the number of organizations and the total number of timepoints within them. Next, summarize the results (e.g., "Lethality climbed as a leader's tenure lengthened, but neither influence style nor power orientation accounted for lethality alone. The two operated in combination: among charismatic leaders, personalized figures ran far deadlier organizations than socialized ones, whereas among pragmatic leaders the pattern reversed. No single power orientation was uniformly more dangerous; its effect depended on the influence style it accompanied."). Finally, draw out the key implications (e.g., "VEOs thus emerge as one of the uncommon settings in which personalized leadership can outperform the socialized orientation that typically prevails, though only when paired with certain influence styles. The findings also suggest that leadership-decapitation strategies might be sharpened by weighing a leader's influence style and power orientation rather than position alone."). Please feel free to adapt these sentences as you see fit.

INTRODUCTION

As Reviewer 1 notes, some repetition in the introduction could be trimmed. We also agree that the "Theoretical Framework" could be folded into the preceding sections, since those already lay out the theory you build on.

More importantly, a substantial gap remains in the introduction. You describe the CIP types and the power orientations clearly in separate sections, which works well. What is missing is a third section devoted explicitly to their interaction. Because the paper rests so heavily on that interaction, it is important to set out, far more concretely, why the two should be expected to interact on theoretical grounds (to be sure, we don’t ask you for any predictions – your work is exploratory, but we want theorizing for why the combination may be of importance). Please add a dedicated section to do so. Within it, it would help greatly to present a 3 (leader type) by 2 (power orientation) matrix as a table or figure, with each cell describing that type of leader and the consequence it might have on a VEO's lethality.

Please attend to some terminological inconsistency. For example, "influence style" vs. "influence tactics" vs. "leadership style". It seems that these terms are used interchangeably throughout the paper, but they are not synonyms within the theoretical framework on which the paper is built.

The introduction focuses a great deal on "outstanding leaders" as a theoretically category. But it seems to be missing from the rest of the paper without explanation in the method and results.

METHOD

We thank you for the several robustness checks, additional analyses, and methodological details supplied in the supplementary materials. Some of these, however, belong in the main text.

Please move Table S4 into the main text and refer to it explicitly there.

We also request some further information. The Kappa statistic you report does show high reliability, but because you are coding two distinct variables, two Kappa values are needed, one for each coding. As requested previously, please state exactly how many leaders were excluded owing to disagreement (see Reviewer 2 as well).

The heading "Research Design and Analysis" should be a subheading nested within Methods rather than a top-level heading of its own. Please also provide the complete coding scheme in the supplementary materials to ensure reproducibility.

Regarding this passage, "Where the data warranted further description, we estimated separate PS × log(time) models within each CIP stratum to characterize the simple PS effect at each level of influence style.", please use emmeans to test these effects on the full model rather than on a subsetted one. The emmeans package handles such contrasts readily, even within more complex interaction structures.

It is stated in the response letter that the authors moved to a “slightly larger available sample” of 68 leaders (up from 56 in the original submission), and that this involved “sacrificing balancing across org structure and culture.” The manuscript does not explain this change to a reader who may not read the reviews. Please describe in a bit more detail what was sacrificed and discuss whether the expanded sample differs from the original in any theoretically relevant way.

RESULTS

Your response on controls is not yet sufficient. We appreciate the test for ideology. Most importantly, though, we also asked for a control for organization size (see Reviewer comments as well), which was neither provided nor adequately justified in its absence. In addition, declining to include controls on the basis of other research or of intuition (you write "we feel that…") and arguing that other factors matter more is insufficient: if those factors are indeed irrelevant, then including them should not change the results. Please address this point more comprehensively. To be clear, whether the controls change your main effects of interest will not affect our decision on the manuscript; it simply speaks to the robustness of the effects, which is important to communicate. At a minimum, please control for group size and present the tests of culture, ideology, and group structure that you already considered in the main text. These could be gathered in a dedicated "Robustness Checks" section at the end of the results.

The comparison of the four functional forms of time should be summarized briefly in the main text, with the details remaining in the SOM. The showcase how thorough you approached this issue.

We asked you to visualize the interactions in place of Table 3. Please do so, and be sure to include 95% confidence intervals in the visualization. Showing this cross-over interaction graphically will make the results far easier for a broad readership to grasp.

Related to the emmeans comment above, you need to report contrast tests. On lines 426 to 434 you describe several differences without statistically testing them, or at least without reporting those tests in the text. This is essential and is readily done with the emmeans package.

In page 20, height of power" is used in the results section to describe the time point at which predicted means are evaluated. But the response letter explicitly states that "height of power of the organization was not relevant to this analysis." The phrase should be removed from the results and replaced with something neutral like "approximately 4 years into a leader's tenure."

DISCUSSION

The null main effects are not discussed before the interaction. The discussion jumps straight to interpreting the interaction without pausing to acknowledge what the null main effects mean. It would be good to add some reflections in this regard.

Please add a paragraph noting that your findings appear to explain a VEO's general effectiveness rather than its trajectory over time. This is a substantial and interesting result that currently receives little attention. We would love to hear more of your reflections regarding this striking finding.

In the limitations section, please address the limits on causal inference. For instance, could reverse causality be at work, with lethality shaping how a leader adjusts?

STYLISTIC

Please replace "non-Western" with an alternative such as "Global South".

p. 70, missing word: "THAT relate".

Please avoid contractions throughout the text (e.g., "doesn't").

The reference list and citations still contain numerous problems. These include mismatched years (e.g., "House, 1997"), an incorrect number of authors (e.g., "Hunt et al., 1999"), commas set incorrectly under APA 7 (e.g., "Hunter, et al. 2025"), in-text citations that never appear in the list (e.g., "Bedell-Avers et al., 2009", "Bedell-Avers et al., 2006"), and entries in the list that are never cited (e.g., "Crayne, M. P., & Medeiros, K. E. (2021)"). These are only a handful of many. Most can be caught with our free tool at https://apa.advances.in, which we already asked you to use in the previous round. The tool is probabilistic and may raise occasional false positives, which you can safely disregard, but the great majority of its flags point to genuine inconsistencies. Please resolve each and every one. There will be no room for oversights of this kind in the next round.

In closing, we very much like the direction of this manuscript and consider it close to ready. With these remaining points addressed through targeted revision, we believe the paper can reach the level we envision for this special issue in the next round. That will depend on the care you bring to our comments, but given the considerable progress already shown, we are confident it will. Thank you again for submitting this important work to our special issue. We look forward to receiving your revision within one month.

Best,

Jonas R. Kunst & Milan Obaidi

Reviewer 1

I commend the authors on the improvements made to the manuscript. I nevertheless have a few remaining observations that may further enhance its quality. The part I believe still needs substantial work is now the Introduction.

Abstract

Please be more specific, particularly in the Results statement of the Abstract. At present, the Abstract only states that “the results of this research suggest that the impact of leadership style on lethality over time depends on the power orientation of the leader,” which is too general. Consider briefly summarizing the key findings to provide readers with a clearer understanding of the study’s contributions.

In addition, the Abstract feels somewhat repetitive, as several sentences reiterate the study's aim or research question. Streamlining these statements and allocating more space to the key findings and their implications would strengthen the Abstract.

Introduction

This section has improved, but it still feels somewhat unfocused and could be streamlined substantially.

For example, lines 196–199 briefly revisit how influence style is shaped by early-life experiences. Given that the origins of influence style are not central to the present study, there may be little need to develop this point earlier in the Introduction (lines 123–130). Instead, the narrative could focus more directly on aspects that are relevant to the current analysis, such as the argument that leadership styles remain relatively stable throughout a leader's tenure (line 200).

The section could also progress more logically by reducing repetition. For instance, the statement in lines 203–204 that "charismatic, ideological, and pragmatic leaders differ in how they exercise influence" largely repeats material already covered in the previous discussion of power orientation. Rather than restating established concepts, the text could more clearly articulate how these ideas lead to the present hypotheses.

More broadly, I wonder whether the separate "Theoretical Framework" section is necessary. Much of its content appears closely connected to the preceding discussion, and integrating the two sections might improve the overall flow and reduce redundancy.

Methods

1. The manuscript again notes that some leaders were excluded due to disagreement between evaluators, but the number of cases removed is not specified. This should be clarified for transparency.

2. The definition provided (p. 317) describes leaders as either acting for the betterment of followers or institutions regardless of personal consequences, or as acting in a personalized manner to acquire, maintain, and enhance personal power. This formulation appears to rely on intentionality and anticipated future outcomes. However, the Introduction emphasizes attribution of authorship of past outcomes (e.g., personalized leaders attributing organizational success to their own authority and centrality; Ligon et al., 2008). As noted in my previous review, the alignment between the conceptualization of power orientation in the Introduction and its operationalization in the Methods section remains unclear and should be better reconciled.

3. I am not able to evaluate the justification for not using a zero-inflated model (line 341), as this is outside my methodological expertise.

4. The rationale for log-transforming time (log(time + 1); line 359) is described in the response to reviewers but is not currently reported in the manuscript. This justification should be included in the Methods section for transparency. Similarly, the explanation of effect coding (line 365) would benefit from an explicit reference to prior methodological literature supporting its use.

5. The statement at line 385 that "substantive findings are like the main model results" is unclear and should be rephrased for precision.

6. While these methodological details are important, some details of the Methods section and the "Descriptive and model-selection results" section may be better placed in supplementary materials to improve readability and focus in the main text.

Results and Discussion

The Results section now flows more naturally, and the Discussion adequately engages with the relevant literature to contextualize and interpret the findings.

Reviewer 2

Overall, I appreciated the significant efforts made by the authors to address the concerns raised on the previous draft. The concerns that I raised in that earlier draft mostly concerned improved reporting in the methods and results sections and the proper specification of the models used for analyses. Those concerns were all sufficiently addressed in this revision, and this draft includes a much more robust analysis procedure. I just have a few points for clarification and/or consideration.

1. There is a typo on p. 19 line 392—there are only four candidate models

2. I believe it is standard practice to also report the R^2 (marginal and conditional) as evidence of model fit. This could be provided for all four models but should at least be provided for the primary model 3.

3. The interactions are described in text as if all comparisons were statistically significant – i.e., personalized leaders were significantly more/less lethal than socialized leaders within a given CIP category. Given that only one of the interaction terms was significant, it would be good to have clarification as to whether this is correct. Is it possible to run pairwise comparisons to test if the differences are significant? I assume the personalized vs. socialized differences are significant in the charismatic and pragmatic conditions, but it was not clear if they were also significant in the ideological condition.

4. Although the authors provided greater theoretical framing to the introduction, that framing does not appear to provide specific predictions for the two-way interactions. I think the authors' choice of stating a research question and not specific hypotheses is consistent with this. I suggest the authors edit that section of the introduction to clarify the exploratory nature of the research given the lack of specific predictions.

5. I found it fascinating that the two largest groups of leaders – socialized charismatics and personalized ideologues – were also some of the least "effective" leaders in terms of lethality. This suggests that there is a considerable mismatch in the power orientations employed by charismatic and ideological leaders of VEOs, but not in the power orientations used by pragmatic leaders. I was hoping to hear the author's thoughts as to why this might happen. It is an interesting finding that was not directly address in the paper, and I wonder if readers would, like me, also appreciate discussion of it.

Round 3

Editor

Date: 2026-07-22

Dear authors,

Thank you for your continious attention to the comments raised in this review process. As a result, the paper can now be published as part of our special issue. Congratulations on a fine contribution to the field! It addresses a blind spot in the field and we believe it will receive considerable attention.

Best,

Jonas R. Kunst & Milan Obaidi